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Abstract

This paper argues that corporate financial frictions can have an adverse effect on employee mental health, an important determinant of employee productivity. To identify the causal effects of financial frictions, we exploit variation in firms' need to refinance their long-term debt in 2008, a period when refinancing became more difficult due to the credit crunch. Using administrative microdata, we find that antidepressant use grows significantly more among employees of firms in higher need of debt refinancing. Much of this effect occurs at employees keeping their jobs, pointing to decreased perceptions of job security as a transmission channel.

JEL codes: G01, G21, I12

Keywords: Financial crisis, financial constraints, credit supply, mental health, job insecurity, job loss, depression, anti-depressants.

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Corporate Financial Frictions and Employee Mental Health

Abstract

This paper argues that corporate financial frictions can have an adverse effect on employee mental health, an important determinant of employee productivity. To identify the causal effects of financial frictions, we exploit variation in firms' need to refinance their long-term debt in 2008, a period when refinancing became more difficult due to the credit crunch. Using administrative microdata, we find that antidepressant use grows significantly more among employees of firms in higher need of debt refinancing. Much of this effect occurs at employees keeping their jobs, pointing to decreased perceptions of job security as a transmission channel.

1. Introduction

A growing literature documents that financial constraints amplify the adverse effects of economic shocks on firms' human capital. Giroud and Mueller (2017) provide evidence that high-leverage firms decreased their employment more during the Great Recession in response to local demand shocks. The authors argue that leverage may impair firms' ability to retain temporarily unnecessarily employees (labor hoarding), a practice that firms may otherwise find optimal in order to preserve human capital and avoid hiring/rehiring costs. Caggese, Cuñat and Metzger (2019) find that financial constraints prompt firms experiencing economic distress to implement sub-optimal dismissal policies, firing short-tenured workers with high future expected productivity. Baghai, Silva, Thell, and Vig (2016) document that firms lose workers with the highest cognitive and noncognitive skills due to financial distress as they approach bankruptcy, whereas Brown and Matsa (2016) show that financial distress can discourage talented job applicants.

In this paper, we document a novel cost of financial constraints on firms' human capital: we provide evidence that financial constraints can exacerbate the adverse effects of economic shocks on employee mental health. Employees' mental well-being should be a primary concern

for any firm, given its role in employee productivity, absenteeism and employee turnover (Bubonya et al., 2017; Burton et al., 2008; Duijts et al., 2007). We argue that financial constraints contribute to a greater probability of job loss, and that decreased job security may trigger mental health problems also for employees who manage to keep their jobs.

To study the effects of financial constraints on employee mental health, we must overcome two empirical challenges. First, we need to establish a quantitative measure of mental health. To do so, we exploit rich administrative data from the Netherlands, in particular a population-wide medicine use register, which records annual binary indicators of medicine use grouped by 4-digit ATC (Anatomical Therapeutic Chemical) codes. As a measure of mental health, we focus on the use of antidepressants (ATC: N06A), drugs that are predominantly prescribed to treat serious mental illnesses, such as depressive disorders, anxiety disorders or bipolar disorders (Gardarsdottir et al., 2007; Simon et al., 2014). Although antidepressant use does not cover the complete spectrum of mental health problems, especially milder conditions, general practitioners in the Netherlands frequently employ antidepressants as the first line of treatment for mental health complaints.[1] Furthermore, as the medically unjustified use of antidepressants is reported to be low (Piek et al., 2011), patients prescribed these medicines indeed suffer from mental problems.

The second empirical challenge is how to disentangle the effects of financial constraints from the effects of economic distress that make these constraints bind. As the papers cited in the introductory paragraph also highlight, the adverse effects of financial constraints on human capital are the most pronounced in bad economic times. Yet, during economic distress, variables that could serve to measure a firm's financial health (such as profitability or firm leverage) are also

[1] In 2010, 30% of patients with any psychological diagnoses were prescribed antidepressants (Nuijen et al., 2012).

likely correlated with the firm's sensitivity to the economic shock, the firm's labor demand, and ultimately the mental health of its employees.

Therefore, instead of focusing on contemporaneous measures of financial health, we identify a balance sheet vulnerability that made firms more likely to be financially constrained during a subsequent economic shock. In particular, we exploit the unforeseen credit supply shock presented by the Global Financial Crisis and employ an empirical strategy motivated by Almeida, Campello, Laranjeira and Weisbenner (2011)[2]. We consider the long-term debt maturity structure of 352 large Dutch companies that employed over 330,000 people on the 1st of January 2008, and identify firms as financially constrained if they had to refinance a large part (minimally 25% in our baseline model) of their long-term debt outstanding in 2008 (we call these firms the *high-repayment* or *treated* firms*)*.

The underlying idea of this identification strategy is that firms that had to repay a larger share of their outstanding long-term debt in 2008 faced refinancing difficulties due to the credit crunch. We offer two pieces of evidence in support. First, bank lending is the main source of external financing for Dutch firms (Kalara and Zhang, 2018), and the Netherlands experienced a strong negative bank credit supply shock in 2007-2008 (Duchi and Elbourne, 2016). As Figure 1 reveals, almost all Dutch banks tightened their lending standards (for large firms) *in each quarter* starting from end-2007, which contributed to the slowdown of business lending observed from mid-2008 (DNB, 2009; van der Veer and Hoeberichts, 2016). In the last quarter of 2008, the net borrowing of Dutch firms turned significantly negative for the first time in many years (Figure 2). Second, there is direct survey evidence indicating that firms experienced a negative credit supply

[2] A similar methodology was applied in several recent papers (e.g. Benmelech et al., 2019; Carvalho, 2015; Duval et al., 2020)

shock: in 2009Q1 21% of the Dutch companies reported the unavailability of bank lending as the most important crisis-related problem they faced (56% among those firms that reported any problems).[3]

[Insert Figures 1 and 2]

As the maturity profile of long-term debt is the cumulative outcome of hard-to-reverse decisions made several years prior to 2008, it is unlikely that the 2008 repayment share is correlated with the sensitivity of the firm to the economic downturn or other unobservable factors. This is particularly true because in our regression models we control for time-invariant employee unobservables (employee fixed effects) and we allow for different flexible time trends for firms with distinct pre-crisis characteristics (controls * year fixed effects). The included control variables (industry, firm size, cash ratio, long-term debt to assets, and cash flow) aim to pick up any systematic differences in firms' long-term debt maturity structure that might have also affected the firm's economic perspectives and personnel policies, and thus its employees' mental health, during the crisis.

The results from the regression models suggest a significant and persistent effect of the credit supply shock on employee mental health. People employed on 1 January 2008 by firms with at least 25% of their long-term debt maturing in 2008 faced a 0.44 pp (percentage points) higher average probability of antidepressant use in the 2008-2012 period, which is an economically significant 9% increase with respect to the 5% unconditional prevalence. The 9% increase in the

[3] COEN Business Survey Netherlands 2009Q1, administered by Statistics Netherlands. The sample consists of establishments with more than 5 employees; the average sample size of the COEN surveys is approximately 6,000 establishments. The crisis-related questions were first added in 2009Q1. The question of interest asks about the *most important* effect of the economic downturn that the respondent experiences (problems acquiring credit, problems attracting equity, losses on deposits, value loss of investments, increased debtor risk or problems saving surplus funds), 62% mentions that none of these effects are important (i.e. no important effects or important effects are unlisted). 21% mentions problems acquiring credit (22% for establishments with over 100 employees), 11% mentions increased debtor risk.

probability of antidepressant use is comparable in magnitude to the 7.5% rise in antidepressant prescription volume due to a 20% decline in US housing prices between July 2006 and February 2009 as estimated by Lin, Ketcham, Rosenquist and Simon (2013).

These results are qualitatively robust to variations in control variables, restricting or broadening the sample of firms, and to altering the 25% refinancing cut-off. We also perform a placebo test to verify that our results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock).

The estimated 0.44 pp increase in antidepressant use is a weighted average treatment effect on employees who left their job during the sample period (*leavers*) and on those who stayed in their jobs (*stayers*). Based on the literature, we argue that a main transmission channel from refinancing difficulties to employee mental health is job loss for *leavers* and decreased job security for *stayers.* Financial constraints may negatively affect firms' labor demand (Benmelech et al., 2011; Chodorow-Reich, 2014; Giroud and Mueller, 2017; Huber, 2018; Popov and Rocholl, 2018), and the ensuing job loss can have an adverse effect on employees' mental health (Browning and Heinesen, 2012; Ganster and Rosen, 2013; Schaller and Stevens, 2015). However, decreased job security can damage employee mental health even in the absence of actual job loss (Burgard et al., 2009; Kim and Von Dem Knesebeck, 2015; Reichert and Tauchmann, 2011; Witte, 1999). Green (2011) concludes that for an employee of average employability the mental health effect of extreme job insecurity is similar to the effect of unemployment.

We indeed find that employees of high-repayment firms had a 6.2 pp higher probability of job separation in the 2008-2010 period. Although we cannot distinguish involuntary job loss from voluntary job separation in our dataset, we show that prior to 2008 employee turnover was similar

in high-repayment firms and in control firms, suggesting that the increase in job separation from 2008 may have been involuntary.

Can a greater propensity of job loss in treated firms completely explain the increase in antidepressant use? We argue that this is not the case, and that *stayers* also suffered from deteriorating mental health. First, in a back-of-the-envelope calculation, we multiply the job loss estimates (with an upper bound of 6.2 pp) with the effects of job loss on depression/anxiety reported by Schaller and Stevens (2015) (1.6 pp). From this calculation it is clear that the 0.44 pp overall increase in antidepressant use is too big to be explained by greater job loss alone. Second, we restrict our sample to employees who kept their jobs at least till the end of the year in which we measure antidepressant use. In this sub-sample, we still find that the probability of antidepressant use in treated firms was 0.28 pp higher in the 2008-2012 period.[4]

Finally, we study treatment heterogeneity among *stayers* to test whether job insecurity is indeed a driver for greater antidepressant use for these employees. Based on the economics and psychology literatures we identify five personal/household characteristics that are expected to increase the mental health burden of job insecurity: older age, being male, having no partner, having children in the household, and having a salary that constitutes a large share of total household income. When we interact our treatment indicator with these moderator characteristics, we find statistically significantly larger treatment effects for employees without a partner, those with children in their household, and for employees whose salary constitutes a large share of their total household income. Treatment effects appear to be larger for employees who are at least 45

[4] The group of employees who keep their job is a selected sample and selection is possibly endogenous to changes in mental health outcomes. For example, employees who stayed with financially constrained firms might be in general more resilient to job insecurity. These employees might have reacted more mildly to increasing job insecurity due to the economic downturn even in the absence of financial constraints, introducing a downward bias in our estimates of the financial constraints' effects on these employees' mental health. As we lack any good instruments for job separation, we cannot claim a causal interpretation for our results on the sub-sample of employees who keep their job.

years old, but the difference is not statistically significant at any conventional level, whereas male and female employees appear to be similarly affected. Taken together, these results provide support to our hypothesis that greater job insecurity is driving increased antidepressant use among employees who do not lose their jobs.

This article relates to three strands of literature in finance and economics. First of all, as cited in the introductory paragraph, a growing literature in finance studies the effects of financial constraints on firms' human capital. We combine firm-level financial data with rich employee-level data on antidepressant use to document a novel cost of financial constraints, their detrimental effect on employee mental health. We show that the mental health toll of financial constraints is not restricted to dismissed employees but it is also substantial for employees who stay with the firm. As argued above, the mental health of employees, particularly of those not dismissed, should be a prime concern of firms due to mental illnesses' burden on employee productivity.

Another strand of literature related to our work studies the health effects of financial and economic crises. Several papers in this field report a negative correlation between unemployment rates and mental health status (e.g. Bradford and Lastrapes, 2009; Charles and DeCicca, 2008; Tefft, 2011). We also study how employment relations contributed to the mental health of employees during a crisis period, but contrary to the previous literature, we use employer-employee matched data to disentangle the mental health effects of the financial crisis (credit supply shock) from the effects of the ensuing economic crisis (the Great Recession). Furthermore, we show that crisis periods may have an adverse mental health effect even on employees who manage to keep their jobs but who may suffer from decreased perceptions of job security.

Finally, this paper also relates to the literature on the health effects of job displacement. Findings of this literature generally indicate a negative causal relation between job loss and mental health (Browning and Heinesen, 2012; Schaller and Stevens, 2015), although not unequivocally (Salm, 2009). The key difference between these papers and our work is that while the job displacement literature's main interest is the effect of job loss per se, we focus on the effects of a firm-level financial shock that may be propagated by job loss, among other channels. We argue that it is not possible to infer the mental health effects of the financial constraints that we study directly from the job displacement literature, most importantly because the majority of employees in financially constrained firms do not lose their jobs, yet they may suffer from workplace stress and increased job insecurity.

The rest of this paper is organized as follows. Section 2 describes the data and the institutional setting, and presents our empirical strategy. Section 3 documents the baseline results, financial constraints' effects on employees' antidepressant use. Section 4 studies a transmission channel, increased job insecurity, and presents evidence that the increase in antidepressant use is not restricted to employees losing their jobs. Section 5 presents robustness and placebo tests. Section 6 concludes.

2. Data, institutional setting, and empirical specification

We use administrative data from the Netherlands. Our dataset combines medicine use and employment data at the individual level with financial data at the corporate (employer) level. All administrative data are provided by Statistics Netherlands (SN), and separate databases are linked using unique (pseudonymized) identifiers at the individual or firm level. Appendix A provides details on the databases used (Table A.1.) and on variable definitions (Table A.2.).

2.1 Firm-level financial data

Under the data framework of Statistics Netherlands, the definition of a firm is hierarchical, whereby the *enterprise group* stands on top of the hierarchy and is considered the center of financial decision making. All corporate financial data are provided at the (consolidated) enterprise group level. An enterprise group consists of one or more *business units*, which are characterized by independent production decisions and the ability to offer their products to external parties, and comprise one or more legal entities (e.g., BVs - private limited liability companies) over which the enterprise group has majority control. An enterprise group in our sample consists of on average 5.5 business units, although 115 of the 352 enterprise groups only have a single business unit. Hereafter we use firm and enterprise group interchangeably.

Firm-level financial data is from the Annual Statistics of Finances of Large Enterprises (SFLE, in Dutch: *Statistiek Financiën van Grote Ondernemingen, SGFO*), which contains information on the consolidated balance sheets and income statements of the largest Dutch enterprise groups. In 2007, all enterprise groups with at least EUR 23m in total assets were surveyed, amounting to a sample of 1204 firms. The scope of consolidation is the Netherlands; foreign subsidiaries of Dutch internationals and Dutch subsidiaries of foreign internationals are not consolidated. Financial data is presented by calendar year; only for a small share of companies does the financial year not coincide with the calendar year.[5]

[5] We do not observe firms' financial years, but as per Bureau van Dijk Orbis data only 9% of the 900 firms in the Netherlands that meet our broadest sample selection criteria (Dutch-owned with at least EUR 23 million in assets) had their 2010 financial year ending *not* on the 31st of December. The financial year is labelled as the calendar year with which it has the most overlap (e.g. data of a financial year ending on 30 September 2019 are labelled as 2019 data).

In most of our regression specifications, we add (industry * year) fixed effects. We use the first two digits of the 1993 version of the Dutch industry classification codes (SBI), which aligns with the European NACE Rev.1 classification at the 4-digit level.[6]

2.2 Employee-level labor data

Information on employer-employee links is provided to SN by the Employee Insurance Agency (EIA or, in Dutch, *UWV*), an administrative authority responsible for implementing employee insurances and recording labor market data. From these data, SN creates the databases *BAANKENMERKENBUS,* which records qualitative job characteristics (e.g., the type of the job such as regular employment or internship, and the start and end dates of an employment relation), and *BAANSOMMENTAB,* which records quantitative job characteristics (such as salaries). We use the information in these databases to link employees to firms, to construct our sample (e.g., excluding interns), and to determine when employees separated from their initial job (using the unique employment relationship identifier – *baanid*).

2.3 Individual-level antidepressant use data

The Netherlands has a universal health care insurance system where taking out the basic health insurance is mandatory for all residents. Care consumers are free to choose among multiple nation-wide private health insurers who offer the same regulated basic insurance package for an annual premium of approximately 1000-1200 EUR (subsidies are available for low-income households). The package covers general practitioner (GP) care, maternity care, hospital care,

[6] We use the codes provided in the General Business Register (in Dutch: *Algemeen Bedrijven Register* or *ABR*). The industry classification codes are registered at the Chamber of Commerce for each legal unit. In the General Business Register, SN provides a code at the business unit level by using the code of the legal unit within the business unit that has the most employees. Similar to this approach, we use the code of the business unit with the most employees within an enterprise group as the enterprise group-level code.

home nursing care, pharmaceutical care, and mental healthcare, but does not cover for example dentistry or physical therapy which may be covered by supplementary insurance products. Care consumers must pay for their health consumption up to an annual deductible (EUR 150 in 2008 and EUR 385 as of 2016; the deductible can be voluntarily increased to lower the insurance premium). Certain care products such as GP care and maternity care do not count towards the deductible.

The initial point of contact for most medical complaints is the general practitioner. All residents are registered with a local GP of their choice. GPs play a gatekeeper role; their referral is necessary for (non-urgent) hospital and specialist care. This holds for mental health problems as well; patients first approach their GP (or in rare cases a so-called first-line psychologist), who may refer them to the second-line specialist mental care in case of any serious problem. GPs frequently employ antidepressant medication as the first line of treatment for mental health complaints; in 2010, 30% of adult patients with *any* psychological diagnoses were prescribed antidepressants (Nuijen et al., 2012). Unjustified antidepressant use is reported to be low (Piek et al., 2011).

The individual-level medicine use database (*MEDICIJNTAB*) comprises annual binary indicators for the use of medicines that are reimbursed under the Dutch basic health insurance scheme. The indicators are grouped at the 4-digit ATC (Anatomical Therapeutic Chemical) level. Therefore, we observe if a person was reimbursed (any positive amount of) antidepressants (ATC-code N06A) in a particular year, but we do not observe the exact chemical substance (e.g., paroxetine, N06AB05) nor do we observe the exact amounts (e.g., defined daily doses, DDDs).

As antidepressants are only available on prescription, and all antidepressants are reimbursed under the basic health insurance[7], the database gives a complete picture of antidepressant use.

2.4 Other databases

We use two additional databases provided by SN in the selection of our employee sample. First, we determine employees' age and gender using the Municipal Personal Records Database (or in Dutch: *Gemeentelijke Basis Administratie* or *GBA*). Second, we collect information on the position of each person in their household from the Income of People (in Dutch; *Integraal Persoonlijk Inkomen* or *IPI*) database.

In order to illustrate the strong (cross-sectional) correlation between worrying about job loss and antidepressant use, we also use answers to the question "Are you concerned of keeping your job?" from the National Labor Conditions Survey (in Dutch: *Nationale Enquête Arbeidsomstandigheden, NEA*), an annual survey on working conditions, accidents at work, work content, and industrial relations.

2.5 Attrition

The administrative databases on medicine use and employment do not suffer from the attrition problems that surveys usually face (e.g., non-response). Yet, attrition might occur if someone leaves the Dutch population, for instance due to emigration or death. Using the Wealth of Households (*VEHTAB*) dataset, which lists all households and household members that belong to the Dutch population on the 1st of January of each year, we find that attrition is similar for treated

[7] Reimbursements for medicines count towards the compulsory annual deductible. Some medicines are only partially reimbursed and a personal contribution must be paid. We could also have studied the use of anxiolytic drugs (N05B, such as benzodiazepines including alprazolam/Xanax); however, starting from 2009 these drugs are only reimbursed in rare cases and are consequently missing from the *MEDICIJNTAB* database. Furthermore, due to the side-effects of benzodiazepines, Dutch guidelines on pharmacological treatment of anxiety disorders recommend the use of antidepressants (Sonnenberg et al., 2012).

and control employees.[8] Attrition affects the definition of our main outcome variable (antidepressant use): we assign a missing value to person-year observations where the given person was missing from *VEHTAB* (we do this because *MEDICIJNTAB* does not cover the antidepressant use of people who are not part of the Dutch population).

2.6 Sample composition

The starting point of our sample selection is the 1204 firms (enterprise groups) in the 2007 annual SFLE (Statistics of Finances of Large Enterprises). Because repayment obligations of local subsidiaries may have limited financial consequences as corporate groups can meet these obligations through their internal capital markets (e.g. Desai et al., 2004), we exclude Dutch subsidiaries of foreign internationals. We identify these subsidiaries as firms with more than 50% of the share capital owned by foreign-based companies or with an Ultimate Controlling Institutional Unit (UCI) located outside the Netherlands.

Following Almeida et al. (2011), we also exclude firms with a low long-term debt (excluding the current portion of long-term debt) to total assets ratio at the year-end of 2007. This is because our treatment classification aims to contrast firms with comparable debt profiles, for which long-term debt financing is a permanently important source of funds. In the baseline specification, we only consider firms with at least 10% of long-term debt to total assets. In robustness tests, we will vary this cut-off.

Finally, we exclude firms that operate in government-controlled and heavily regulated industries. These are government management (SBI code 7511), public transport via railway (6010), national post with universal service obligation (6411), and utilities (40-41). We further

[8] Untabulated; by 2016 treated employees are 0.16% more likely to be in the Dutch population (t-stat 0.74).

exclude outsourcing firms (74501, 74502) because we cannot observe the actual company where outsourced employees work. The resulting sample consists of 353 firms (enterprise groups).

We identify the business units of these enterprise groups on 1 January 2008 using the General Business Register (ABR). Subsequently, for the same date, we identify the people employed by each business unit. We restrict our sample to employees with a regular or on-call job contract[9] aged between 20 and 60 years in 2008[10], and who are the household head of their household or the partner of the household head[11]. The final sample consists of 328,229 employees. The steps of sample composition are presented in Table 1.

[Insert Table 1]

2.7 Treatment classification and summary statistics

We classify firms as "treated" or "control" based on the share of long-term debt that they were required to repay in 2008. Unlike most databases comprising European firms' financial data (e.g., Bureau van Dijk's Orbis), which report the current portion of long-term debt aggregated with all other current liabilities, the SFLE database reports these items separately. This is important because other current liabilities, such as short-term bank loans, may be correlated with the business

[9] On-call employees only work when the employer calls them up, as they do not have fixed working hours. We exclude employees classified as interns and outsourced workers. We also exclude director-major shareholders, who are people with a considerable ownership in the firm they manage.

[10] In the sample period, early retirement was widespread in the Netherlands with 80% of employees retiring before reaching the state pension age of 65, mostly at the age of 60. As labor market shocks have arguably limited impact on employees close to retirement, we exclude them.

[11] SN classifies people as either household head (person with the highest socio-economic position), partner (married or unmarried) of the household head, children of the household head, or other/unknown (e.g. children of the partner from a previous marriage). We only include people of the first two categories, excluding children and other/unknown household members because we aim to limit our sample to people for whom an employment shock (or a threat thereof) has high stakes.

outlook that the company faced preceding the crisis, and may thus fail to be exogenous to the outcomes we study. We calculate our "forcing" variable as

$$Share\ of\ current\ portion\ of LT\ debt = \frac{Current\ portion\ of\ LT\ debt}{Current\ portion\ of\ LT\ debt\ +\ Total\ LT\ debt}$$

where Total LT debt is the part of long-term debt maturing beyond one year.[12] In our baseline specification, we classify firms as treated if the "*Share of current portion of LT debt*" ratio is at least 25%. This results in 23 treated and 329 control firms. In robustness tests we will vary this cut-off point.

Table 2 presents summary statistics for treated and control firms (panel A) and for their employees included in our sample (panel B). The last three columns show a comparison between treated and control; the column "Raw Δ" presents the difference in means, the column "Adjusted Δ" presents the difference in means adjusted in a regression setting for industry fixed effects (and in Panel B also for the firm financial controls that we include in our baseline regressions: log total assets, liquid assets to total assets, long-term debt to total assets, and cash flow), and the last column presents the significance of the regression coefficient "Adjusted Δ" by means of a t-statistic.

[Insert Table 2]

[12] The SFLE database differentiates between five categories of long-term debt, (1) Debt to group companies, (2) Subordinated loans, (3) Bonds outstanding, (4) Loans from domestic financial institutions and (5) Other long-term debt (a residual category that includes, inter alia, loans from private individuals, derivatives, and lease obligations). Ideally, we would only consider bonds outstanding and bank loans (and the current portion thereof) because these financing forms are the hardest to renegotiate. However, the SFLE reports the current portion of all five debt categories combined. Although the scope of this problem is limited as bank loans constitute by far the largest share of long-term debt for most sample companies, in Table 8 we also execute a robustness test where we exclude firms with any long-term intercompany debt on their 2007 opening balance sheet (the type of long-term debt presumably the least binding).

As Panel A shows, treated firms (at the end of 2007) were slightly smaller in terms of total assets but larger in terms of number of employees (column Raw Δ). Treated firms had a somewhat lower cash ratio (liquid assets to total assets) and more long-term debt outstanding relative to their total assets, but they exhibited a more positive cash flow in 2007. As the "Adjusted Δ" and "t-stat" columns reveal, once controlling for industry composition, only this latter difference (and the difference in our forcing variable, share of current portion) is statistically significant at the 5% level. We control for any (remaining) difference in these variables in our regression models.

Turning to employee characteristics in Panel B, *antidepressant use*, our main outcome variable, is an annual binary indicator that takes the value 1 if a person was reimbursed for antidepressant medications in the given year (we multiply the indicator by 100 hence our results are in %). 4.1% of our sample used antidepressants in 2007, comprising 4.75% of treated employees and 3.99% of control employees. This difference practically disappears in the regression setting in column "Adjusted Δ". While a similar pre-treatment *level* of the dependent variable is not required for identification in a difference-in-differences setting, it is reassuring that the included control variables adequately explain any differences in treated and control employees' antidepressant use in 2007. Regarding other employee characteristics, treated and control employees have similar tenure, although treated employees are slightly younger and more likely to be female. Once controlling for industry fixed effects and the financial control variables, in column "Adjusted Δ", these difference in age and gender diminish and even reverse. In a robustness test we will also control for year effects interacted with these employee characteristics to account for any time trends that might depend on these characteristics (e.g., older employees might be more affected by the crisis).

While Table 2 presents the pre-treatment characteristics of treated and control employees, we present the mean of our binary outcome variables (antidepressant use, job separation) at the bottom of the respective regression tables.

2.8 Empirical specification

Our empirical specification compares the time-trend of antidepressant use of employees of high-repayment firms (treated) and other sample employees (controls), accounting for employee fixed effects, industry-specific year effects, and year effects that depend on pre-crisis firm (or employee) characteristics. We estimate a linear probability model:

$$\text{Antidepressant Use}_{i,f,j,t} = \alpha_i + T_f\beta_t + \gamma_{j,t} + x'_{f,2007}\lambda_t + \epsilon_{i,f,j,t} \quad (1)$$

where $Antidepressent\ Use_{i,f,j,t}$ is a binary indicator variable capturing whether individual *i* who worked on 1 January 2008 for firm *f* belonging to industry *j* was reimbursed for any antidepressant use in year *t*, α_i are employee fixed effects, T_f is the treatment indicator that takes the value 1 for treated firms and 0 for control firms, $\gamma_{j,t}$ are (year * industry) fixed effects, and $x_{f,2007}$ is a 4-by-1 column vector of firm *f*'s 2007 financial characteristics, comprising log total assets, liquid assets to total assets, long-term debt to total assets, and a measure of cash flow [=(net income plus depreciation and amortization) / total assets].[13] These financial characteristics are derived from Almeida et al. (2011), who argue that industry fixed effects and these financial characteristics[14] capture a lot of otherwise unobserved firm heterogeneity that is important both for the treatment classification (i.e., maturity structure of long-term debt) and firms' business conditions prior to

[13] In a robustness test (column 4 of Table 4) we also include in x a set of 2007 employee characteristics to account for the differences between treated and control employees reported in Panel B of Table 2.

[14] Almeida et al. (2011) further control for Tobin's Q and credit ratings, variables unavailable in our dataset.

and during the crisis. β_t are the differential year effects for the treated firms, our main coefficients of interest. We estimate Model (1) using data from 2006 to 2013, equivalent to 2 years prior to and 5 years following the 2008 financial crisis. Due to the presence of individual fixed effects we normalize β_{2007} to 0. We cluster standard errors at the firm (enterprise group) level because the treatment variation is at the firm level.

We quantify the average treatment effect over the 2008-2012[15] period using a difference-in-differences model:

$$\text{Antidepressant Use}_{i,f,j,t} = \alpha_i + T_f \; Post \; \beta + x'_{f,2007} \; Post \; \lambda + Post \; \gamma_j + \epsilon_{i,f,j,t} \qquad (2)$$

where $Post$ is an indicator for the post-treatment period (2008-2012), γ_j are industry fixed effects, $x_{f,2007}$ is a 4-by-1 column vector of the same 2007 firm financial characteristics as in Model (1), and consequently λ is a 4-by-1 column vector of coefficients. The included periods are 2006 to 2012.

In order to study treatment heterogeneity, we also use a version of Model (2) where we interact the treatment indicator with pre-treatment employee characteristics:

$$\begin{aligned}\text{Antidepressant Use}_{i,f,j,t} &= \alpha_i + \; z'_{i,2007} \; T_f \; Post \; \beta + T_f * Post \; \delta_1 + \; z'_{i,2007} * Post \; \delta_2 \\ &+ \; Post \; \gamma_j + x'_{f,2007} \; \text{Post} \; \lambda + \epsilon_{i,f,j,t}\end{aligned} \qquad (3)$$

where $z_{i,2007}$ is an n-by-1 column vector of 2007 employee characteristics such as age, gender or having a partner and β is an n-by-1 column vector of coefficients.

[15] We opt to quantify the average treatment effect for the 2008-2012 period as the estimation of Model (1) will reveal that treatment effects in 2013 are not statistically significant anymore.

3. The effect of financial constraints on employee mental health

Figure 3 presents the estimated treatment effects (β_t) from Model (1). Employees of the treated firms, relative to employees of the control firms, experience an increase in antidepressant use starting from 2008; the treatment effect reaches its peak in 2011 and it is not statistically significantly different from zero in 2013 anymore.

[Insert Figure 3]

The relatively fast increase in antidepressant use in 2008 may reflect increased job insecurity (as discussed in Section 4), and is in line with the findings of Schaller and Stevens (2015) who document that displaced US workers exhibit depression or anxiety within months after the loss of their jobs. The immediacy of the treatment effect is further supported by results from the psychology literature. Kendler et al. (1999) study 15 different stressful life events and find that 11 of them, including job loss, and financial or housing problems, are significantly associated with the onset of major depression *in the month of occurrence*. The onset of depression may have a swift effect on antidepressant use due to the prescription preferences of Dutch general practitioners: Van Marwijk, Bijl, Adèr, and De Haan (2001) report that in 1998 Dutch GPs prescribed antidepressants in 73% of the *first* consults for depressive symptoms.

The persistence of the treatment effect can be partially explained by the persistence of depression. Depression (medical term: major depressive disorder) is a lifelong illness that is categorized by recurrent depressive episodes. The majority of patients recover (i.e., are no longer symptomatic) within 12 months following a depressive episode; however, long-term recovery (lack of recurrence) is low, approximately 30% at a 6-year horizon, and almost 80% of patients experience at least one further episode in their lifetime. Furthermore, a large proportion (up to 27%) of patients never recover and develop chronic depressive illness (Malhi and Mann, 2018). The

long-lasting nature of depression is also supported in our medicine use data, 57% of people in our sample who used antidepressants in 2006 continued to do so in 2012.

We also estimate treatment effects for 2006, to investigate "parallel trends" before the treatment. Ideally, we would present trends for multiple pre-treatment periods, but the medicine use database is only available from 2006. As Figure 3 illustrates, treated and control employees demonstrated a similar change in antidepressant use between 2006 and 2007, conditional on the control variables.

The coefficient estimates from Figure 3 and the corresponding standard errors are presented in Table 3. The table also shows the estimates on the control variables (interacted with the year indicators). Employees of firms with higher long-term debt to total assets ratio in 2007 exhibited a greater increase in antidepressant use during the crisis, whereas employees of larger firms exhibited a smaller increase. Both the 2007 cash ratio (liquid assets to total assets) and cash flow appear to diminish growth in antidepressant use, although these estimates are mostly not statistically significant.

[Insert Table 3]

Table 4 presents the average 2008-2012 treatment effects from Model (2), a difference-in-differences model. All specifications control for employee fixed effects. The baseline specification in column (1) further controls for (industry * year) fixed effects and year fixed effects that depend linearly on 2007 firm financial characteristics. Columns (2) to (4) present variations on these additional controls. Column (2) drops the (2007 firm financials * year) fixed effects, while column (3) defines industries at a coarser (sectoral) level instead of using 2-digit Dutch SBI93 industry codes. Finally, because the descriptive statistics in Table 2 show that treated and control employees exhibited some pre-treatment differences in age, salary, and tenure, column (4) includes the

interaction of these characteristics with year dummies. All four specifications show qualitatively similar results, with 2008-2012 treatment estimates ranging between 0.26 pp and 0.47 pp (a 5.4% to 9.7% effect relative to the 4.8% baseline probability of antidepressant use).

[Insert Table 4]

To better understand the magnitude of these treatment effects, we can compare them to estimates from the literature on the mental health effects of wealth and employment shocks. Our main result of a 5.4% to 9.4% relative increase in the probability of antidepressant use due to firm-level (re)financing difficulties is similar to the effect of the 2006-2009 US housing price shock (7.51% rise in antidepressant prescription volume) reported by Ketcham, Rosenquist and Simon (2013), but smaller than the effect of job loss (22% increase in the probability of depression/anxiety) calculated by Schaller and Stevens (2015) or the effect of losing on average USD 220,000 during the October 2008 market crash (35% relative increase in the probability of antidepressant use) estimated by McInerney, Mellor and Nicholas (2013). This benchmarking exercise shows that firm-level (re)financing difficulties had a serious impact on employee mental health, although not directly comparable to the effects of job loss.

4. The transmission channel of job insecurity

4.1 Increased job insecurity of treated employees

How can firm-level refinancing difficulties lead to an increase in employees' antidepressant use? The estimated 0.44 pp increase in antidepressant use is a weighted average treatment effect on employees who left their job during the sample period (*leavers*) and on those who stayed in their jobs (*stayers*). We argue that an important transmission channel from

refinancing difficulties to employee mental health is job loss for *leavers* and decreased job security for *stayers*.

Previous work demonstrated that financial constraints can negatively affect firms' labor demand. Chodorow-Reich (2014) finds evidence, among 2,000 non-financial US firms, that companies reduced employment more in the 2008-2009 period if they had pre-crisis relationships with banks that were in a less healthy condition during the financial crisis. Huber (2018) shows that German firms fully dependent on Commzerbank, a bank severely affected by the 2008 financial crisis, reduced their employment on average by 5.3% between 2009 and 2012 compared to firms with no Commerzbank relationship. Giroud and Mueller (2017) report that employment in more highly levered US firms was more sensitive to declines in local consumer demand during the Great Recession. Giroud and Mueller argue that financing constraints may dampen labor demand by impairing firms' ability to engage in *labor hoarding*, a practice of retaining temporarily unnecessary employees to preserve firm-specific human capital and to avoid firing/re-hiring costs.

During the first crisis years, labor hoarding was widespread in the Netherlands; indeed, several studies credit to this phenomenon the relatively mild increase in Dutch unemployment rates between 2008 and 2010 (e.g. van den Berge et al., 2014). Nonetheless, as highlighted by Giroud and Mueller (2017), labor hoarding requires financial resources, which are scarcer for financially constrained firms. In the Netherlands, financial resources are particularly important for labor hoarding due to the inflexible employment terms regarding both working hours and wages. Over 80% of Dutch employees are covered by collective labor agreements (CLAs), which largely prevent companies from adjusting nominal wages downwards. Adjustments in the number of working hours are also not straightforward to implement because Dutch CLAs, unlike for instance German ones, do not contain provisions for temporary shorter working hours (Tijdens et al., 2014).

Given these observations, we hypothesize that firms that had to repay a larger share of their long-term debt in 2008 had relatively fewer resources to engage in labor hoarding, and consequently employees of these firms suffered from decreased job security. The adverse mental health effects of job loss are well-documented (Browning and Heinesen, 2012; Ganster and Rosen, 2013; Schaller and Stevens, 2015), which could explain treatment effects on *leavers*. However, decreased job security can damage employee mental health even in the absence of actual job loss (Burgard et al., 2009; Kim and Von Dem Knesebeck, 2015; Reichert and Tauchmann, 2011; Witte, 1999).[16] Green (2011), for instance, concludes that for an employee of average employability the mental health effect of extreme job insecurity is similar to the effect of unemployment. Therefore, decreased job security could also explain treatment effects on *stayers*.

In line with this hypothesis, we find evidence that employees in treated firms were more likely to separate from their job during the crisis period. Table 5 presents the treatment effects on the (cumulative) probability of job separation. We assume that an employment relationship ended in a given year ("job separation") if the job is not registered anymore in the "Quantitative characteristics of employment relationships" database (*BAANSOMMENTAB*) in the following year. We only consider the initial employment relationships that existed on 1 January 2008. The results in columns (1), (2) and (3) show that treated employees had a 4.6, 6.7 and 6.2 pp higher probability of job separation by the end of 2008, 2009 and 2010, respectively. This is an economically significant increase compared to the unconditional means of job separation, for example 30% by end-2010. Job separation is statistically significantly lower in larger firms, whereas it appears to

[16] We also illustrate this negative association between job insecurity and mental health in the Netherlands, using the National Working Conditions Survey. Although we cannot establish causality, Table B.1 in Appendix B shows that employees answering "yes" to the question "Are you concerned of keeping your job?" are substantially (~2 pp or 44% relative to the 4.5% baseline) more likely to use antidepressants, even after controlling for a broad range of personal and firm characteristics.

be larger in firms with higher end-2007 long-term debt to total assets ratio and cash ratio. In columns (4) to (6) we further control for the same pre-treatment employee characteristics that we also controlled for in column (4) of Table 4. After adding these additional controls, the treatment effects increase slightly with treated employees facing a 7.2 pp greater probability of having separated from their job by the end of 2010. Age and tenure statistically significantly decrease the probability of job separation.

[Insert Table 5]

There are two caveats to the estimated treatment effects on job separation. First, it might be that treated firms in general have higher employee turnover, even after controlling for industry fixed effects and 2007 firm characteristics. We cannot study "parallel pre-trends" of job separation in this setup because, by definition, all employees are with their firm on the 1st of January 2008. Second, our definition of job separation includes both voluntary and involuntary departures, which the data do not enable us to distinguish. To address these issues, we study the 2005-2007 job separation rate of *2005* employees of the treated and control firms. First we match the 352 firms in our sample to their employees on 1 January 2005.[17] We then estimate three regressions for (cumulative) job separation up until end-2005, end-2006, and end-2007 using the same controls as in columns (1)-(3) of Table 5 (industry fixed effects and 2007 financial characteristics). Table B.2 of Appendix B presents the results from this placebo test. If anything, the employees of treated firms in 2005 were less likely to be separated from their jobs than employees of control firms, although the estimates are economically and statistically close to zero. This leads us to conclude

[17] We can match 325 of the 352 enterprise groups to their business units and employees on 1 January 2005. Out of the 27 non-matched enterprise groups, 24 could not be matched because their identifier in the General Business Register changed between 2005 and 2008 due to restructuring, split, re-starting or mergers. Two enterprise groups were created in the period, and we find no information on one remaining enterprise group.

that in terms of employee turnover, treated firms were not different from control firms prior to the crisis. Although this test cannot directly address the issue of voluntary vs. involuntary job separation, we have no good reason to expect an increase in voluntary job separation during the crisis period compared to prior years.

In summary, the results in Table 5 provide evidence that, during the first crisis years, job separation was greater in firms that had to repay a larger share of their long-term debt in 2008, and part of this increment in job separation was presumably involuntarily.

4.2 Increased antidepressant use among employees who kept their jobs

Can the greater propensity of job loss and its negative effects on employees who lost their jobs fully explain the deteriorating mental health of employees of firms with refinancing difficulties? We argue that this is not the case, and that employees who managed to keep their jobs also suffered from an increased prevalence of mental health problems.

First, in a back-of-the-envelope calculation, we multiply the job loss estimates (with an upper bound of 6.2 pp) in Table 5 with the effects of job loss on self-reported depression/anxiety estimated by Schaller and Stevens (2015) (1.6 pp). The resulting treatment effect (6.2 pp * 1.6 pp = 0.1 pp) is clearly smaller than the 0.44 pp overall increase in antidepressant use we estimate in Table 4. This suggests that it is not merely mental illness caused by job loss that is driving our results.

Next, in Table 6 we restrict our sample to employees who kept their jobs at least till the end of the year in which we measure antidepressant use. In the first seven columns we repeat the analysis of Table 3 for this restricted sample of employees, while in the last column we estimate the average 2008-2012 treatment effect as in column (1) of Table 4. We observe a similar trend in

treatment effects as for the complete sample, the excess probability of antidepressant use rapidly increases in 2008 and remains statistically significant until 2013. The average 2008-2012 treatment effect amounts to 0.28 pp, or 6.5% compared to the unconditional mean antidepressant use of 4.3% in this sample. Although we cannot interpret these results causally due to the possibly endogenous nature of job separation, discussed in footnote 4, they support a negative effect of refinancing constraints on the mental health of employees who kept their jobs.

[Insert Table 6]

Finally, to test whether job insecurity is indeed a driver for antidepressant use among employees who kept their jobs, we study the moderating effect of personal/household characteristics that are expected to increase the mental health costs of job insecurity. We consider five characteristics based on the economics and psychology literatures, older age, being male, having no partner, having children in the household, and having a salary that constitutes a large share of total household income. Personal characteristics (age and gender) may influence re-employability upon job loss. In the Netherlands, Deelen et al. (2018) show that, following a dismissal older (age 45-54 in their sample), men lose more in terms of employment probabilities than either prime-age men (age 35-44) or older women. The mental health effects of unemployment and job loss also appear to be stronger for men than for women (e.g., Kuhn et al., 2009; Paul and Moser, 2009). This suggests that job insecurity may be more stressful for older and male employees. Having no partner may show the lack of a familial support system and could increase the risk of developing mental illness (Teo, Choi and Valenstein, 2013), whereas having a child could indicate that job loss is more consequential due to the higher number of dependents. Finally, having a salary that represents a greater share of the total household income (conditional on having a partner or not) may imply a more detrimental effect of an eventual job loss on the

family budget. Indeed, Marcus (2013) suggests that the mental health effects of job loss are worse if the dismissed employee had a higher pre-dismissal share of household income.

[Insert Table 7]

Table 7 shows the moderating effects of these characteristics on the 2008-2012 average treatment effect of antidepressant use, based on Model (3). The table presents the coefficient estimates of the triple interactions Post*Treated* Characteristic and also of the double interactions Post*Treated and Post*Characteristic. Columns (1) to (5) show results from five separate univariate specifications where we interact Post*Treated with a single characteristic measured pre-treatment (during 2007 or on 1 January 2008), while Column (6) presents results from a model where all the five triple interactions are included. The results show that treatment effects are larger for employees without a partner (at least in the multivariate specification), who have at least one child in their households, and whose salary constitutes a large share of their total household income. Treatment effects appear to be larger for employees who are at least 45 years old (we adopt this cut-off from Deelen et al., 2018), but the difference is not statistically significant at any conventional level, whereas male and female employees appear to be similarly affected. Taken together, these treatment heterogeneity estimates provide evidence that job insecurity is indeed a potential factor behind the increase in antidepressant use for employees who did not lose their jobs.

In summary, our results suggest that the adverse mental health effects of refinancing constraints are present both for employees who lose their jobs due to these constraints and for employees who manage to keep their jobs albeit possibly suffer from greater job insecurity. From the firm's perspective the mental illness of non-dismissed employees is of particular importance as it may negatively affect firm productivity. Therefore, our results illustrate that the

mental health costs of financial constraints are not restricted to dismissed employees but also affect the firm.

5. Robustness and placebo tests

There are several assumptions regarding treatment specification and sample selection that underpin our results of a greater post-2007 increase of antidepressant use in high-repayment share firms. In this section, we present estimates where we relax/alter these assumptions. We also perform a placebo test to verify that our results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock).

First, in Table 8 we present robustness tests based on changes in the sample selection criteria. Column (1) presents the baseline estimate of the 2008-2012 average treatment effect on antidepressant use from Table 4. Column (2) excludes firms with any long-term debt resulting from intra-group loans on their opening 2007 balance sheet. Ideally, we would restrict our 2008 repayment share variable to the repayment of long-term debt that is most probably binding and hard-to-renegotiate, such as bank loans and bonds. Due to data limitations this is not possible, but excluding firms with intercompany loans would alleviate concerns that our repayment share variable picks up non-binding repayment obligations within the group. The point estimate from column (2) is very close to the baseline estimate, although the sample size decreases.

Columns (3) to (5) investigate alterations on the long-term debt to total assets selection criterion to capture the degree to which firms rely on long-term debt. When we include firms with lower long-term debt to total assets ratios (columns (3) and (4)), the treatment effects become smaller, while restricting the sample to firms with at least 15% long-term debt to total assets

increases the estimate. This is as expected, as the size of the refinancing shock is arguably proportional to the share of long-term debt on the balance sheet.

Column (6) retains all industries (i.e., the state-controlled and heavily regulated industries such as utilities are not excluded). Adding firms that belong to regulated industries or form part of the state administration yields a slightly lower (0.34 pp vs 0.44 pp) 2008-2012 treatment estimate, indicating that for such firms the refinancing problems may be more easily addressed without repercussions on the job security of the employees.

Finally, in columns (7) and (8), we re-estimate the treatment effects after controlling for a firm size effect. We exclude the largest 5% and 10% of firms, respectively, where we measure size as the number of employees in the sample. Excluding these large firms yields almost identical treatment effects, indicating that the results are not driven by a handful of the largest firms.

[Insert Table 8]

Next, we turn to alternative thresholds for the current portion of long-term debt to define treated and control firms. Table 9 repeats the analyses of Table 3 and column (1) of Table 4, and presents the estimated treatment effects on antidepressant use when we use a lower (20%) or higher (30%) cut-off value. The effects for a higher (30%) cut-off, which results in only 10 treated firms, are slightly larger than the baseline estimates. If we choose a lower cut-off value (20%), our estimation results are expectedly attenuated due to the fact that the refinancing (di)stress may be somewhat lower.

[Insert Table 9]

Finally, we perform a placebo test to verify that our results are not driven by the excess sensitivity of the treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic

effects unrelated to the credit supply shock). We exploit the fact that after a short-lived recovery in 2010-2011 the Dutch economy fell back into recession in the second half of 2012 ("double-dip recession"). Importantly, Duchi and Elbourne (2016) show that the effect of credit supply shocks on corporate lending growth and corporate investments is negligible in this period, noting that "when we look at the double-dip recession in 2012, adverse credit supply shocks play no role" (p. 65). Therefore, the 2012 recession presents a negative economic shock without a strong corporate credit supply component: if our results are indeed driven by disruptions in corporate credit supply, we would expect to find no positive treatment effects in this period. Indeed, we demonstrate in Table 10 that, following the 2012 recession, growth patterns in antidepressant use are similar between employees of firms that had to repay a large share (>25%) of their long-term debt in *2012* and employees of other firms in our sample. If anything, employees of high-repayment firms exhibited a slight decrease in antidepressant use. Column (2) repeats the job separation analysis from Table 5 in this placebo setting. The results reveal that employees of 2012 high-repayment firms did not suffer from increased job insecurity. In summary, the placebo test shows that repayment of a high share of long-term debt has no detrimental effects on employees during an economic downturn when credit constraints were not binding.

[Insert Table 10]

6. Conclusion

This paper argued that corporate financial constraints can have adverse effects on employee mental health, and that these effects are not restricted to employees who lose their job due to these constraints. To identify the causal effects of financial constraints, we exploited the plausibly exogenous variation in firms' need to refinance their long-term debt in 2008, a period when refinancing became more difficult due to a severe tightening of bank lending standards. Using

administrative data from the Netherlands on the antidepressant medicine use of 330,000 employees in 352 firms, we estimated that employees of firms that were facing the repayment of at least 25% of their long-term debt in 2008 were 0.44 pp more likely to consume antidepressants in the 2008-2012 period. This is an economically significant 9% increase relative to the 5% unconditional prevalence of antidepressant use, comparable to the 7.5% rise in antidepressant prescription volume following the 20% decline in US housing prices between July 2006 and February 2009 estimated by Lin, Ketcham, Rosenquist and Simon (2013).

These results are qualitatively robust to alternative industry classifications, variations in control variables, restricting or broadening the sample of firms, and to altering the 25% refinancing cut-off. A placebo test suggests that the results are not driven by the excess sensitivity of treated firms to the economic downturn in 2008-2009 (i.e., macroeconomic effects unrelated to the credit supply shock).

Although the estimated effects can be partially explained by higher job loss in constrained firms, much of the increase in antidepressant use occurs at employees who manage to keep their jobs. Studies of employee-level heterogeneity in the treatment effect, among employees who keep their jobs, suggest that antidepressant use grew more for employees for whom job insecurity may represent a greater mental health burden: employees with children, employees without a partner and employees who salary constitutes a greater share of family budget. Although we lack direct data on employee perceptions of job security, these results suggest that increased perception of job insecurity is a transmission channel for deteriorating mental health.

Given the important role of mental health in employee productivity, these results provide evidence that deteriorating mental health represents a hitherto undocumented cost of financial constraints for firms. Furthermore, they also illustrate that crisis periods can have an adverse

mental health effect even on employees who manage to keep their jobs, as these employees may still suffer from decreased perceptions of job security.

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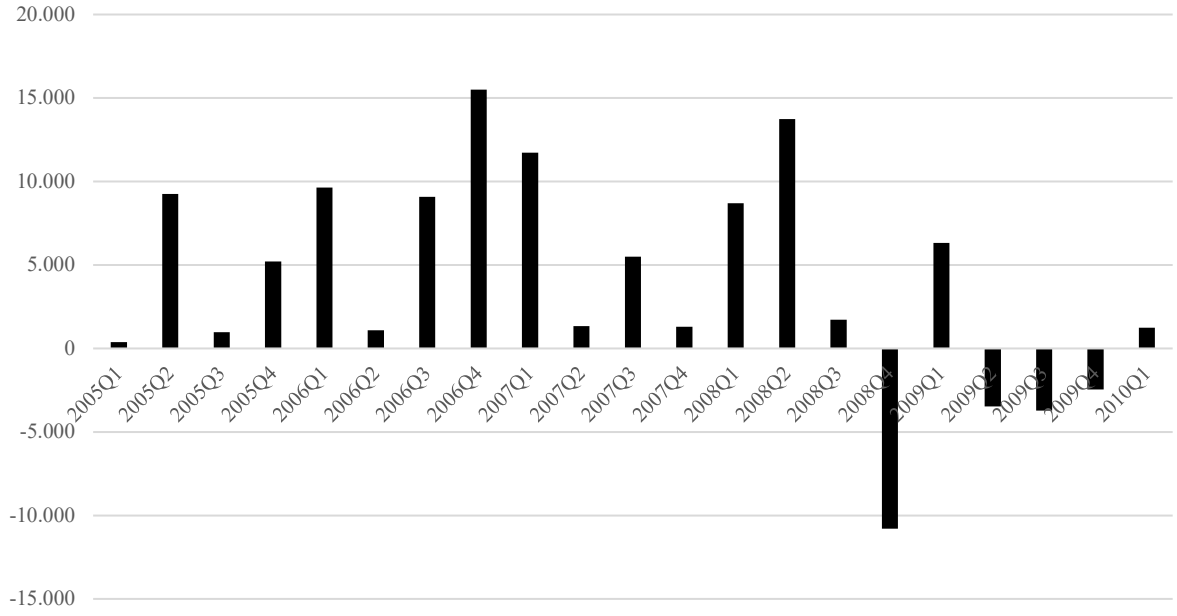
Figures:

Figure 1: Credit standards of Dutch banks on loans to large enterprises

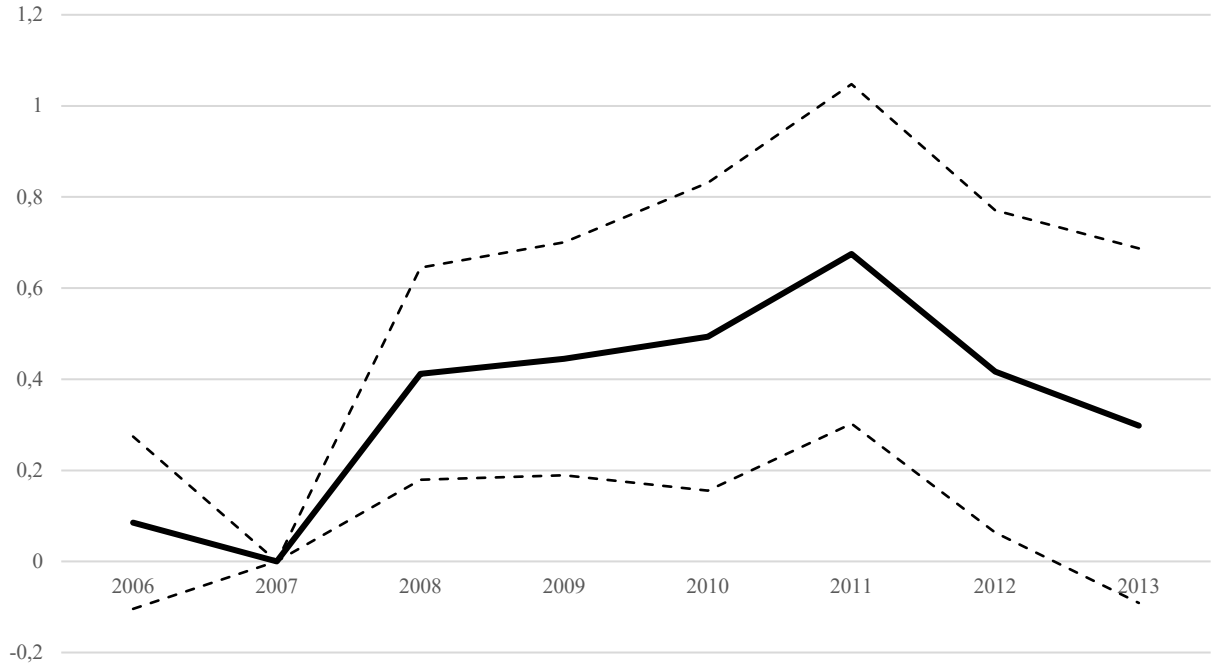
Notes: Net percentages of banks tightening and easing their credit standards (overall) in the preceding quarter, weighted by loans outstanding; Source: ECB (SDW item BLS.Q.NL.ALL.O.E.Z.B3.ST.S.BFNET)

Figure 2: New loans minus retired bank loans of Dutch non-financial companies (EURm)



Notes: Retired bank loans minus new loans of Dutch non-financial companies (EUR mln), Source: Statistics Netherlands - Quarterly sectoral accounts (CBS - *Kwartaalsectorrekeningen*)

Figure 3: Treatment effects on antidepressant use (percentage points)



Notes: Estimated treatment effects on antidepressant use (percentage points) and 95% confidence interval. The corresponding coefficient estimates and t-statistics are also presented in Table 3.

Tables:

Table 1: Steps of sample composition

	# enterprise groups	# business units	# employees
Total in 2007 SFLE	1,204		
Excluding foreign-owned firms	609		
Excluding firms with <10% LT debt on total assets	378		
Merging with business units	378	3,018	
Merging with employees	376	2,106	801,297
Excluding government-controlled and regulated industries	353	1,936	464,447
Restricting to age 20-60 years	352	1,917	388,539
Restricting to household head and partner	352	1,914	331,899
Excluding interns, outsourced employees and director-major shareholders	352	1,899	328,229

Notes: The table presents the steps taken to arrive at the final sample of firms and employees.

Table 2: Pre-treatment summary statistics

	Treated						Control						Raw Δ	Adjusted Δ	t-stat
Panel A: Firm characteristics	**N**	**mean**	**sd**	**p10**	**p50**	**p90**	**N**	**mean**	**sd**	**p10**	**p50**	**p90**			
Liquid assets to TA, 2007	23	0.03	0.04	.	0.00	.	329	0.05	0.08	0.00	0.01	0.12	-0.02	-0.01	-0.93
LT debt to TA, 2007	23	0.43	0.19	.	0.43	.	329	0.32	0.19	0.14	0.27	0.59	0.11	0.07	1.55
Cash flow, 2007	23	0.17	0.13	.	0.14	.	329	0.11	0.09	0.03	0.10	0.20	0.06	0.06	2.12
Total assets, 2007 (EURm)	23	489	1753	.	82	.	329	663	2457	33	92	1139	-174	51	0.11
Share of current portion of LT debt	23	0.34	0.09	.	0.30	.	329	0.06	0.06	0.00	0.03	0.15	0.28	0.27	13.41
No. of employees in sample	23	1552	5554	.	242	.	329	889	1920	70	285	1993	663	909	0.74
Industry composition:															
Wholesale and retail trade	12						88								
Other	11						233								
Panel B: Employee characteristics															
Antidepressant user, 2007 (%)	35692	4.75	21.26	0.00	0.00	0.00	292537	3.99	19.57	0.00	0.00	0.00	0.76	0.00	0.01
Tenure in years, 2008	35692	9.26	8.80	0.00	7.00	22.00	292537	8.79	8.85	0.00	6.00	21.00	0.47	0.99	1.91
Age, 2008	35692	39.60	10.85	24.00	40.00	55.00	292537	42.10	9.98	28.00	42.00	56.00	-2.50	1.46	2.50
Female	35692	0.50	0.50	0.00	0.00	1.00	292537	0.32	0.47	0.00	0.00	1.00	0.18	-0.07	-1.50

Notes: The table reports pre-treatment descriptive statistics for the treated and control firms, and their employees. The column Raw Δ presents the difference in means. The column Adjusted Δ presents the difference in means estimated in a regression where we control for 2-digit SBI 1993 industry fixed effects, and in case of Panel B also for the four financial variables included in our main specification (liquid assets to TA, LT debt to TA, cash flow, and log TA). The column t-stat presents the t statistic on the regression coefficient reported in column Adjusted Δ. No 10^{th} and 90^{th} percentiles are reported for treated firms, following Statistics Netherlands guidelines, because these values would refer to fewer than 10 companies. Variable definitions are presented in Table A.2 of Appendix A.

Table 3: Effects on antidepressant use over time

Dependent variable: antidepressant use (binary, x100)	**2006**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**
Treated	0.0849	0.412***	0.445***	0.486***	0.667***	0.407**	0.295
	(0.88)	(3.47)	(3.39)	(2.81)	(3.52)	(2.25)	(1.47)
Liquid assets to TA, 2007	-0.501	-0.317	-0.505	0.156	-0.892	-1.028	-0.751
	(-1.34)	(-0.97)	(-1.25)	(0.28)	(-1.31)	(-1.34)	(-1.10)
LT debt to TA, 2007	0.0199	0.301	0.731***	0.745***	0.858***	0.606**	0.625**
	(0.12)	(1.53)	(3.17)	(3.23)	(3.55)	(2.20)	(2.27)
Log total assets, 2007	0.00556	-0.0236	-0.0205	-0.0710***	-0.0780***	-0.0766***	-0.120***
	(0.32)	(-1.25)	(-0.91)	(-2.72)	(-2.71)	(-2.74)	(-3.87)
CF, 2007	-0.180	-0.436	-0.723	-0.661	-0.566	-0.397	-0.884
	(-0.45)	(-1.15)	(-1.42)	(-0.86)	(-0.64)	(-0.44)	(-0.99)
Unconditional mean (%)	3.72	4.31	4.58	4.83	5.14	5.33	5.60
Employee f.e.	Yes						
Industry * year f.e.	Yes						
# Firms (=clusters)	352						
# Observations	2,603,121						

Notes: The table shows estimates of the effect of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (1). All columns belong to a single regression; each column shows coefficient estimates on the year * Treated and year * Controls interactions for the given year. 2007 is the omitted year. As specified in Model (1), the regression includes employee fixed effects as well as 2-digit SBI93 industry*year fixed effects. Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. Antidepressant use is only defined for people who lived in the Netherlands on the 1st of January of the given year. The row 'unconditional mean' presents the sample mean of the dependent variable for the given year. Variable definitions are presented in Table A.2 of Appendix A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Average treatment effect for 2008-2012

	Antidepressant use (x100)			
Post * ...	Baseline	No covariates	Coarser industry	Employee covariates
Treated	0.440***	0.260***	0.333***	0.473***
	(3.09)	(3.54)	(2.91)	(3.35)
Liquid assets to TA, 2007	-0.261		-0.141	-0.326
	(-0.56)		(-0.33)	(-0.72)
LT debt to TA, 2007	0.637***		0.627***	0.618***
	(3.48)		(4.12)	(3.40)
Log total assets, 2007	-0.0565***		-0.0538***	-0.0486**
	(-2.66)		(-2.85)	(-2.41)
CF, 2007	-0.470		0.0912	-0.473
	(-0.67)		(0.17)	(-0.72)
Age, 2008				-0.0014
				(-0.50)
Female				0.288***
				(3.81)
Tenure, 2007 (years)				-0.011***
				(-3.41)
Unconditional mean (%)	4.84	4.84	4.84	4.84
Employee f.e.	Yes	Yes	Yes	Yes
Industry * Post f.e.	SBI93	SBI93	Sections	SBI93
# Firms (clusters)	352	352	352	352
# Observations	2,282,057	2,282,057	2,282,057	2,282,057

Notes: The table shows mean 2008-2012 treatment effect estimates of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use, based on Model (2). Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. The interaction of the Treated treatment indicator and the control variables with the Post indicator (which takes the value 0 in 2006-2007 and the value 1 in 2008-2012) are tabulated. All models also control for employee fixed effects and 2-digit SBI93 industry*Post fixed effects. Column (1) presents the baseline specification. Column (2) does not include the Post*firm-level control variables. Column (3) uses a coarser, sectoral-level, industry classification. Finally, compared to Column (1), Column (4) also include pre-treatment employee characteristics interacted with the Post indicator. The row 'unconditional mean' presents the sample mean of the dependent variable. Variable definitions are presented in Table A.2 of Appendix A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Treatment effects on cumulative job separation

	Firm controls			*Firm and employee controls*		
Dependent variable: cumulative job separation (binary, x100)	**2008**	**2009**	**2010**	**2008**	**2009**	**2010**
Treated	4.60***	6.66**	6.20**	5.60***	7.93***	7.47**
	(2.97)	(2.29)	(2.01)	(3.95)	(2.92)	(2.58)
Liquid assets to TA, 2007	4.50	9.26	14.0*	4.50	8.74	13.24*
	(0.84)	(1.22)	(1.69)	(1.01)	(1.36)	(1.85)
LT debt to TA, 2007	3.41	8.64*	8.29	3.38	8.57**	8.25*
	(1.16)	(1.90)	(1.64)	(1.31)	(2.10)	(1.80)
Log total assets, 2007	-0.529*	-0.991**	-1.24**	-0.150	-0.524	-0.769*
	(-1.90)	(-2.10)	(-2.52)	(-0.61)	(-1.19)	(-1.65)
CF, 2007	-3.53	-9.31	-11.4	-7.86	-13.29	-14.33
	(-0.63)	(-0.93)	(-1.02)	(-1.65)	(-1.49)	(-1.43)
Age, 2008				-0.356***	-0.344***	-0.270***
				(-6.51)	(-4.38)	(-2.94)
Female				-0.091	1.443*	1.974**
				(-0.18)	(1.77)	(2.01)
Tenure, 2007 (years)				-0.489***	-0.681***	-0.755***
				(-7.37)	(-7.55)	(-6.93)
Unconditional mean (%)	14	23	30	14	23	30
Industry f.e.	Yes	Yes	Yes	Yes	Yes	Yes
# Firms (clusters)	352	352	352	352	352	352
# Observations	328,229	328,229	328,229	328,229	328,229	328,229

Notes: The table reports treatment effects (coefficient estimates on the treatment indicator) from regression models where the dependent variable is *cumulative* job separation. Cumulative job separation is originally a binary indicator that takes a value 1 if an employee's initial (1 January 2008) job ended, for any reason, by the end of the year in consideration (the year indicated in the header of the column); we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. Columns (1) to (3) control for 2007 firm financial characteristics, whereas columns (4) to (6) further control for 2007 employee characteristics. All regressions include 2-digit SBI93 industry fixed effects. As on 1 January 2008 all employees – by definition – worked at their initial job, the regressions do not include employee fixed effects. The row 'unconditional mean' presents the sample mean of the dependent variable (in %). Variable definitions are presented in Table A.2 of Appendix A. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Treatment effects on employees keeping their job (stayers)

Dependent variable: antidepressant use (binary, x100)	**2006**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**	**2008-2012**
Treated	0.0849	0.323***	0.326**	0.362**	0.342	0.406**	0.144	0.278**
	(0.88)	(2.60)	(2.03)	(1.98)	(1.63)	(2.27)	(0.73)	(2.15)
Liquid assets to TA, 2007	-0.501	-0.226	-0.181	0.129	-0.704	-0.688	-0.899	-0.0897
	(-1.34)	(-0.71)	(-0.37)	(0.22)	(-0.96)	(-0.82)	(-1.51)	(-0.20)
LT debt to TA, 2007	0.0199	0.282	0.745***	0.974***	0.965***	0.469	0.368	0.636***
	(0.12)	(1.30)	(3.18)	(3.63)	(2.98)	(1.35)	(1.08)	(3.24)
Log total assets, 2007	0.00556	-0.00615	-0.00388	-0.0326	-0.0378	-0.0215	-0.0632*	-0.0170
	(0.32)	(-0.30)	(-0.16)	(-1.26)	(-1.19)	(-0.73)	(-1.90)	(-0.81)
CF, 2007	-0.180	-0.273	-0.668	-0.430	-0.155	-0.760	-0.914	-0.280
	(-0.45)	(-0.70)	(-1.10)	(-0.56)	(-0.18)	(-0.94)	(-1.25)	(-0.47)
Unconditional mean	3.72	4.10	4.22	4.30	4.50	4.53	4.67	4.31
Employee f.e.	Yes							Yes
Industry * year f.e.	Yes							Yes
# Firms (=clusters)	352							352
# Observations	1,986,249							1,817,359

Notes: The table shows estimates of the effect of a firm having to repay at least 25% of long-term debt in 2008 (Treated) on employees' antidepressant use. Antidepressant use is originally a binary variable that takes the value 1 if a person was reimbursed for (any) antidepressant use in the given year; we multiply this variable by 100 and therefore all coefficients in the table are expressed in %. Contrary to Tables 3 and 4, which follow employees over time even if they leave their initial job, this table only considers employees who stay in their initial job (the job on 1 January 2008) for at least the end of the year of the observation. The first seven columns present the treatment effects over time, based on Model (1), in a similar manner as in Table 3. The last column presents the average 2008-2012 treatment effect, based on Model (2), in a similar manner as in Table 4 (a difference is that this specification controls for industry * post fixed effects instead of industry * year f.e.). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Treatment heterogeneity among *stayers*

	Antidepressant use (x100)					
Post * ...	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treated	0.252**	0.0239	0.333**	0.183	0.262*	-0.285
	(2.04)	(0.16)	(2.27)	(1.43)	(1.82)	(-1.40)
No partner	-0.288***					-0.279***
	(-3.28)					(-2.82)
Treated * No partner	0.114					0.430**
	(0.87)					(2.41)
Has child(ren)		0.135*				0.0215
		(1.94)				(0.29)
Treated * Has child(ren)		0.336***				0.470***
		(3.04)				(3.66)
Female			0.341***			0.319***
			(4.06)			(3.87)
Treated * Female			-0.0681			-0.00340
			(-0.60)			(-0.02)
High share in household income				-0.113*		0.00155
				(-1.86)		(0.03)
Treated * High share in household income				0.280***		0.379***
				(2.62)		(3.22)
Age above 44					-0.0988*	-0.135***
					(-1.93)	(-2.66)
Treated * Age above 44					0.0512	0.145
					(0.47)	(1.39)
Industry*post f.e.	Yes	Yes	Yes	Yes	Yes	Yes
2007 firm characteristics * post	Yes	Yes	Yes	Yes	Yes	Yes
Employee fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# Firms (=clusters)	352	352	352	352	352	352
# Observations	1,633,679	1,633,679	1,633,679	1,633,679	1,633,679	1,633,679

Notes: The table reports estimates of treatment heterogeneity for the 2008-2012 average treatment effect, based on Model (3). All specifications include the same controls as Column (1) of Table 4. As in Table 6, we restrict observations to *stayers*, employees who keep their jobs at least till the end of the year in consideration. Columns (1) to (5) present results from five univariate specifications where we interact Post*Treated with a single characteristic measured pre-treatment (during 2007 or on 1 January 2008). Column (6) presents a multivariate specification where Post*Treated is interacted with each characteristic. *No partner* is 1 if a person lived without a partner (unmarried or married). *Has child(ren)* is 1 if a person had at least one child in his/her household. *High share in household income* is 1 if the share of a person's salary in his/her total household income was in the top half of the distribution (conditional on having or not having a partner). *Age above 44* refers to the age of a person in 2008. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Variations on sample selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	**Excluding firms with group lending**	**LT debt to TA >0%**	**LT debt to TA >=5%**	**LT debt to TA >=15%**	**No industry restrictions**	**Excluding top 5% firms**	**Excluding top 10% firms**
Average treatment effect 2008-2012 (binary, x100)	0.440***	0.377**	0.225**	0.357***	0.497**	0.338***	0.410**	0.406**
	(3.09)	(2.47)	(2.48)	(3.03)	(2.50)	(4.04)	(2.57)	(2.28)
Employee fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry * year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2007 firm variables * year f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Treated firms	23	22	41	31	20	25	22	21
# Control firms	352	304	485	408	301	375	335	317
# Observations	2,282,057	1,279,136	3,092,926	2,622,039	1,944,757	3,210,309	1,205,793	855,764

Notes: The table reports 2008-2012 average treatment effects (from Model 2) when the sample selection criteria are changed. All specifications include the same controls as Column (1) of Table 4. Column (1) shows the baseline (repeats column (1) in panel B of Table 4). Column (2) excludes firms that had any long-term group lending on their 2007 opening balance sheet. Columns (3) to (5) vary the minimum long-term debt to total assets ratio (excluding the current portion). Column (6) also includes firms from government-controlled and highly-regulated industries. Columns (7) and (8) exclude the top 5% and 10% largest firms (based on the number of employees in our sample), respectively. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Alternative treatment classifications

Dependent variable: antidepressant use (binary, x100)	**(1)**	**(2)**	**(3)**
	25% (baseline)	**20%**	**30%**
Panel A: dynamic effects			
2006	0.0849	0.133	0.0459
	(0.88)	(1.32)	(0.38)
2008	0.412***	0.291**	0.362***
	(3.47)	(2.04)	(3.34)
2009	0.445***	0.270*	0.400**
	(3.39)	(1.66)	(2.56)
2010	0.486***	0.339*	0.561**
	(2.81)	(1.88)	(2.53)
2011	0.667***	0.487***	0.696***
	(3.52)	(2.63)	(2.76)
2012	0.407**	0.274	0.488**
	(2.25)	(1.42)	(1.99)
2013	0.295	0.163	0.198
	(1.47)	(0.80)	(0.77)
Panel B: average effects			
2008-2012	0.440***	0.265*	0.479**
	(3.09)	(1.78)	(2.52)
Employee fixed effects	Yes	Yes	Yes
Industry * year fixed effects	Yes	Yes	Yes
2007 firm variables * year f.e.	Yes	Yes	Yes
# Treated firms	23	37	10
# Control firms	329	315	342
# Observations (Panel B)	2,282,057	2,282,057	2,282,057

Notes: The table presents alternative treatment specifications, variations on the 2008 repayment threshold of long-term debt. Panel A shows the treatment effects over time; column (1) of Panel A corresponds to the first row of Table 3. Panel B shows the average 2008-2012 treatment effect; the estimate in column (1) corresponds to the treatment estimate in column (1) of Table 4. All specifications include the same controls as Table 3 and column (1) of Table 4. In column (2) we classify firms as treated if they had to repay at least 20% of their long-term debt in 2008, whereas in column (3) if they had to repay at least 30% of their long-term debt. The changing number of treated and control firms is presented at the bottom of the table. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Placebo test – firms with high *2012* debt repayment share

	(1)	(2)
	Antidepressant use	**Cumulative job separation**
2010	-0.125	
	(-1.35)	
2012	-0.107	-0.0146
	(-1.30)	(-0.43)
2013	-0.252**	0.000868
	(-2.38)	(0.03)
2014	-0.188	-0.0193
	(-1.34)	(-0.54)
2015	-0.355**	-0.0361
	(-2.58)	(-0.96)
2016	-0.284	
	(-1.43)	
Employee fixed effects	Yes	No
Industry * year fixed effects	Yes	Yes
2011 firm variables * year f.e.	Yes	Yes
# Firms (clusters)	406	406
# Observations	2,485,867	1,433,912

Notes: The table presents the results of a placebo test where we define financially constrained (treated) firms as those that had to repay at least 25% of their long-term debt in *2012*. Column (1) presents treatment effect estimates on antidepressant use, as defined in Table 3; the coefficient estimates on the treatment indicator * year interaction terms are shown. The omitted year, due to employee fixed effects, is 2011. Column (2) presents treatment estimates on cumulative job separation, as defined in Table 5. Control variables are similar to those in Table 3 and Table 5, respectively, but are defined using 2011 data. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e., enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix A:

Table A.1: Statistic Netherlands datasets used

Name in English	SN name	Description
Annual Statistics of Finances of Large Enterprises, SFLE	Statistiek Financiën van Grote Ondernemingen, SGFO	Annual survey on the finances (balance sheet, income statement) of the largest non-financial enterprises in the Netherlands. As of 2007, all enterprises are sampled with total assets over EUR 23 million. Close to 100% response rate for the largest 300 enterprises.
General Business Register, GBR	Algemeen Bedrijven Register, ABR	Continuously updated database of companies registered in the Netherlands, with information on corporate/legal structure (enterprise group, business units, legal entities), industry classification codes and events (e.g. mergers, liquidation).
Qualitative characteristics of employment relationships	BAANKENMERKENBUS	Information on, inter alia, start and end date of employment relationship, type of employment (e.g. regular employee, on-call, outsourcing, manager-large shareholder), social security insurance indicators (e.g. insured for unemployment benefits).
Quantitative characteristics of employment relationships	BAANSOMMENTAB	Information on, inter alia, taxable salary, calendar days worked and payroll tax withheld.
Annual dispensations of medicines per ATC-4 code per person	MEDICIJNTAB	All medicines dispensed that are reimbursed under the basic health insurance policy to persons who are registered in the Municipal Personal Records Database (GBA). No quantities are recorded; merely the 4-digit ATC codes (e.g. N06A) are listed that were dispensed for a given person in the statistical year.
Extract from the Municipal Personal Records Database	Gemeentelijke Basis Administratie, GBAPERSOONTAB	Demographic background data (that do not or hardly change) of all persons who appear in the Municipal Personal Records Database from 1 January 1995 (e.g. gender, year of birth, migration background).
Income of People	IPI	Annual income components (such as labor income, subsidies, income from entrepreneurship) of people resident in the Netherlands on the 1st of January of the statistical year. Information on the position of the person within the household with respect to the head of the household.
Wealth and household composition	VEHTAB / KOPPELTABELVEHTAB	Annual wealth components (assets and liabilities) of households in the Netherlands on the 1st of January of the statistical year. *KOPPELTABELVEHTAB* contains information on household members.
National Labor Conditions Survey	*Nationale Enquête Arbeidsomstandigheden*, NEA	Annual survey of workers (excluding self-employed) between 15 and 74 years old on working conditions, work content, labor relations and employment conditions.

Table A.2: Variable descriptions

	Variable name	Additional note	Source/variable
Long-term debt structure	Long-term debt to group companies	Both in the Netherlands and abroad, maturity>1 year	SFGO /B65
	Subordinated loans	Maturity>1 year	SFGO /B67
	Bonds outstanding	Maturity>1 year	SFGO /B69
	LT bank loans	Loans from domestic financial institutions, including mortgages, maturity>1 year	SFGO /B71
	Other long-term debt	Other unclassified long-term debt, including loans from private parties, financial leasing, derivatives, member loans (for cooperatives)	SFGO /B73
	Current portion of long-term debt	Repayment obligation of long-term debt (including bonds and other debt) due within one year	SFGO /B85
	Total long-term debt	= Long-term debt to group companies + Subordinated loans + Bonds outstanding + Loans from domestic financial institutions + Other long-term debt	
	Total long-term debt including its current portion	= Current portion of long-term debt + Total long-term debt	
	Share of current portion of LT debt	= (Current portion of long-term debt) / (Total long-term debt including its current portion)	
Firm characteristics	(Log of) total assets		SFGO/B37
	Liquid assets to total assets ratio	Liquid assets are the sum of Cash and cash equivalents, Term deposits with financial institutions, and Receivables from financial institutions (current account). The ratio is defined as (Liquid assets)/(Total assets)	SFGO/B31-B35; SFGO /B37
	Long-term debt to total assets	= Total long-term debt / Total assets	
	Cash flow	= (Net income + depreciation and amortization) / Total assets	SFGO/R20, R05
	SBI93 - 1993 version of the Dutch industry classification codes	The industry classification codes are registered at the Chamber of Commerce for each legal unit (e.g. B.V.). In the GBR, SN provides a code at the business unit level by using the code of the legal unit within the business unit that has the most employees. Similar to this approach, we use the code of the business unit with the most employees within an enterprise group as the enterprise group level code.	ABR / RBE_SBI93
Employee characteristics	Initial job	The job (employment relationship) that existed on 1 January 2008 and based on which the employee was selected into the sample (employees with multiple jobs on 1 January 2008 are excluded)	
	Tenure in years	Integer part of number of days since the employment relationship exists (on 1 January 2008) divided by 365 (e.g. tenure in days = 400, tenure in years = 1).	BAANKENMERKENBUS / DATUMAANVANGBAANID
	Age in 2008	= 2008 – year of birth	GBAPERSOONTAB / GBAGEBOORTEJAAR
	Female		GBAPERSOONTAB / GBAGESLACHT
	Has a partner	Takes the value 1 if person i is recorded as household head with (married or unmarried) partner, or as partner of the household head in the 2007 Income of Households dataset; otherwise takes 0.	IPI / POSHHK

	Variable name	Additional note	Source/variable
Outcome variables	Antidepressant use indicator	Takes the value 1 if person i is listed as an antidepressant (ATC4 code: N06A) user in year t. Takes the value 0 if person i is not registered as antidepressant user and person i is in the supplementary table (KOPPELTABELVEHTAB) of the Wealth of Households (VEHTAB) dataset, which contains all residents on 1 January. The variable is set to missing otherwise.	MEDICIJNTAB, KOPPELTABELVEHTAB
	Cumulative job separation	Takes the value 1 if the initial job of person i terminated by the end of the given year. A job is considered terminated in year t if there is no salary received from the job in year t+1 (more precisely if the job identifier *baanid* cannot be matched to year t+1's BAANSOMMENTAB datafile); otherwise equal to 0.	BAANSOMMENTAB / BAANID

Notes: This table reports the description of all the variables used in the analysis. The Annual Statistics of Finances of Large Enterprises (SFLE) contains both opening and closing balance sheet values; in the main analysis we use 2007 closing values, therefore we refer to these variables in the table below. "Initial job" refers to the employment relationship that existed on 1 January 2008, the date on which employees were matched to employer firms in our sample. All monetary values are in nominal EURs.

Appendix B:

Table B.1: Worrying about job loss and antidepressant use

Dependent variable: antidepressant use (%)	**(1)**	**(2)**
Concerned about keeping job	2.190***	1.843***
	(9.36)	(7.88)
Female		2.375***
		(11.99)
Age		0.519***
		(8.30)
Age squared		-0.00447***
		(-5.89)
Has partner		-2.354***
		(-9.25)
Tenure in years		-0.0492***
		(-4.15)
Constant	4.118***	-8.332***
	(41.52)	(-6.86)
2-digit SBI 93 industry fixed effects	Yes	Yes
Unconditional mean antidepressant use (%)	4.53	4.53
# Firms (clusters)	21436	21436
# Observations	61575	61575

Notes: The table presents the relation between antidepressant use and the indicator "Concerned about keeping job" that is based on the question "Are you concerned about keeping your job? (yes/no)" from the National Labour Conditions Survey (NLCS). Antidepressant use is defined as in Table 4. In both specifications a pooled cross-sectional regression is estimated with data from 2007 to 2010. Column (1) only controls for industry fixed effects, whereas column (2) further controls for gender, age, age squared, an indicator if the person lived with a (married or unmarried) partner, and the length of the person's current employment relationship (on the 1st of January of the year). The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.2: Job separation in treated firms between 2005 and 2007

	Job separation 2005	Job separation 2006	Job separation 2007
Treated	-0.00585	-0.00891	0.00374
	(-0.23)	(-0.36)	(0.14)
Liquid assets to TA, 2007	-0.0668	-0.0497	-0.0463
	(-1.42)	(-0.75)	(-0.51)
LT debt to TA, 2007	0.0432	0.0964**	0.151**
	(1.43)	(2.24)	(2.11)
Log total assets, 2007	-0.000283	-0.00690**	-0.0112**
	(-0.11)	(-2.37)	(-2.56)
CF, 2007	0.0578	0.101	0.0812
	(0.90)	(1.16)	(0.73)
Industry fixed effects	Yes	Yes	Yes
Unconditional mean of outcome	0.133	0.232	0.325
# Enterprise groups (clusters)	325	325	325
# Observations	275714	275714	275714

Notes: This table presents estimates of differential job separation rates between firms defined as Treated in our baseline specification (23 firms that had to repay at least 25% of their long-term debt in 2008) and firms defined as Control, controlling for 2-digit SBI93 industry fixed effects and financial variables measured year-end 2007. The sample includes 1 January 2005 employees of the Treated and Control firms who meet the same sample selection criteria as for Table 4 (on-call or regular job, between 20 and 60 years old, household head or partner of household head). The dependent variable (job separation) is defined as in Table 6. The t-statistics, reported in parentheses, are based on standard errors clustered at the firm (i.e. enterprise group) level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively